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On the Improvement of Life Contingency Calculation. By EDWIN JAMES FARREN, ESQ., *one of the Vice Presidents of the Institute of Actuaries.*

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THE prevailing system of life contingency calculation is one not of variable but of invariable quantities. At the very threshold the admission of two such important assumptions is asked for, as that the rate of mortality is always invariable at the same age, whether old or young, and that the rate of interest is equally invariable for all periods, whether long or short. Upon these admissions of invariability a system is formed for assessing the relative values of different cases, thereby necessarily in every instance indicating an invariable answer; and with such indications the system rests content. Whether such assessments, however logically fair in connection with agreed postulates of invariability, are themselves eventually justified by the same invariability of actual result as was *à priori* assumed, has not hitherto been commonly brought within the general scope of the actuary's studies. Directly, however, he is called on to take upon himself the practical responsibility of upholding this theory of invariability, he is somewhat surprised to find that, good as the mere logic of his studies may have been, it is by no means an easy task to connect such logic with the nature of the events he may see passing around him. In place of a definite rate of mortality at each age, he may find a perfect series

of such rates—in place of one uniform rate of interest, he may find one portion of the funds yielding no interest whatever, while other portions may be lent upon classes of security, fruitful indeed in interest, but not as equally fruitful in obvious expedients for realization of the principal, should such be desirable. The conviction is thus forced upon him at a very early period of his career, that, as a practical man, he must either henceforth shut his eyes altogether to the prevailing theory of the subject, or else open his eyes much wider than as a student he was taught to expect would be necessary in the application of rules professedly based upon invariable elements.

I am aware it may be said, that although the rates both of mortality and interest, as used by actuaries under the prevailing system, are certainly specific rates, yet that they are to be considered as averages, and therefore typical, as well in theory as in practice, of a diversity of rates. But I would ask, has the subject ever been so treated? Can we find, for instance, in the treatises of those who are termed by common consent, and therefore rightly termed, our standard writers, not merely a single chapter, but even a single page, on the proper calculation and employment of average results, as applicable to insurance transactions? So long then as this omission exists, so long I think we may without presumption assert that the prevailing system of life contingency calculation is susceptible of considerable improvement. But in alluding to this omission, I particularly wish to guard myself from the inference that I thereby desire to stigmatize the labours of such men as Price, Morgan, Baily, and Milne, or of our late excellent cotemporary David Jones, as not worthy of the favourable reception they have received. On the contrary, I believe (and it is one of the objects of the present paper to illustrate such belief) that the style of treatment adopted by these pioneers of our literature constituted the only style that was really applicable to the nature of insurance, as then publicly understood. Indeed, in all branches of natural philosophy we find a similar assumption of invariability at an early period of their history, as preparatory to the more comprehensive study of deviations required by modern science. One of these branches presents us with so close a parallelism to the line of progress I wish to illustrate as advisable in insurance calculations, that I cannot consider it wholly a digression to allude to it even in some detail.

In the early history of navigation, we find it taken almost as the basis of the science that the compass needle pointed in a fixed

direction, and that such direction was due north. The utility of so simple an assumption in early days can scarcely be overrated; the more especially as it was by this simplicity then rendering itself acceptable that the real properties of the magnet have been reserved for more elaborate study in our own time. Indeed, had variability in place of invariability been originally taken for its prevailing attribute, the important uses of the mariner's compass would probably have been lost to modern science, inasmuch as our simplicity-loving ancestors might have considered such indications too uncertain and complex either to be useful to themselves or to their posterity. In further illustration of this, we find that for a long time the simplicity of such a theory of invariability completely overrode the nature of the actual facts. Gradually, however, the proverbial stubbornness of facts developed itself in this as in other sciences, and brought about the admission that the compass needle did not really and in fact point due north. There was as yet, notwithstanding, no absolute surrender of the principle of fixity of direction itself; such would have been too sudden a change either to have been expected or even desired. A variety of adjustments were accordingly advocated, each claiming some favourite point as that of the real fixity. The correct theory, nevertheless (started, I believe, by Gunter, and now admitted by all conversant with the subject, because the only theory that can interpret all the facts), is, that the direction of the compass needle is variable even in the same localities, and must be so apprehended by mariners, if safety based upon truth, and not merely upon simplicity of theory, be their aim. It is, then, by the known variability of the compass needle, and not by a pseudo invariability, that the triumphs of modern over ancient navigation have been achieved.

The use I seek to make of this as a parallelism in illustration of the subject before us is, I presume, sufficiently obvious. I consider the formerly universal adoption of the Northampton Table and 3 per cent. as typified by the assumption that the compass needle pointed fixedly due north, and the various petty controversies for other fixed points as equally typified by the various pros and cons for the Carlisle and other tables. Further, that the true theory, in this as in the former subject, is one strictly of variation, both as to mortality and interest, and that it must be so accepted as the only guide to safe practice, if we would avoid those rocks and shoals which a purblind adherence to a fixed in place of a variable course might unpreparedly develope. Indeed, the distinction between the proper treatment of variable and invariable ele-

ments is precisely the distinction that characterizes the vocation of an actuary as compared with that of an accountant. Thus the actuary who should take probabilities, because fairly assessed now, as necessary certainties hereafter, would be virtually an accountant, because he allows no range for the possibly conflicting evidences of the future. The accountant, on the other hand, who endeavours to put estimates upon fluctuating things to come, is virtually striving to be an actuary; for he cannot but allow that no estimate of to-day, unless professedly subject to variation, can pretend to also fulfil the condition of being an equally good estimate for a changeable to-morrow. In some Offices, I believe, this distinction between fixed and variable estimates is already sufficiently carried out—in the first case, by the actual amount of assets at one period, as compared with the actual amount at another, being illustrated by DR. and CR. after the manner of accountants; and in the second, by the difference between the amount of the life valuation at one period and another being substantiated, actuary-wise, by taking into consideration the accrued contingencies of the past as compared with the range of contingencies to be provided against for the future. The actuaries, then, of what I shall venture to call the old school, were essentially accountants in the modern sense, for it was only with fixed quantities they professed to deal, as is sufficiently proved by their assuming a fixity when they found it not.

The results of such a system have been exactly those to be expected. Where exorbitancy secretly existed, as in the rates required by the Insurance Societies, there the errors of a fixed and affected precision eventually came to light, in the shape of bonuses added to the sums assured, in varying amounts from time to time, in strange contrast with the declared formality of the original fixedness of calculation. Where no such exorbitancy of charge was allowable or even possible, and the fixed calculations had to stand or fall by their own merits, there the dangers of professing to deal with variable quantities as if they were absolutely invariable were unfortunately not so easily neutralized. To the numerous Friendly and Annuity Societies of the last century throughout the country, a reliance on fixed tables and on such tables alone has at once proved a delusion and a snare; for it tempted them to appropriate the temporary surplus of a day, in the vain expectation that the fixed nature of the tabular values assigned to the future would necessarily be sufficient guarantee for the fulfilment of impending engagements.

The nature, then, of the improvement I seek to introduce into

life contingency calculation is to openly take as our guide not merely a calculus of averages, but of their fluctuations; and to thereby declaredly characterize our methods, not as composing a system of specific and precise *results,* whatever it may be of *prices,* but of results expected to vary between limits of assigned ranges of probability. By such a declaration it would at once become manifest that our expected gain by computation would not be to find even averages themselves invariable, but that their fluctuations, being considerably less, would therefore be more readily dealt with than the fluctuations of the elements of which they may be composed. The phrases therefore of the prevailing system implying "a true table," or "a true rate of interest," would under such a calculus have to give way to average tables, with their probable limits and the per centages of their expected deviations.

What experienced actuary, for instance, can read without feeling the truth of the following reflection, extracted from the article on "Probability" in the *Encyclopædia Metropolitana*:—

"Not being well able to decide upon the relative importance of small details, calculators on this subject (life contingencies) have hitherto judiciously presented their results such as they ought to be if the tables were mathematically exact, and to the nearest farthing. But more extended views on the subject of probabilities, and on the nature of observations in general, would have caused the time which has been wasted in carrying out annuities to many more decimals than the data are good for, to be employed in apportioning the risks of fluctuation by estimation of the mean risks of the tables."

Or the following, from the equally excellent article on "Probability" in the *Encyclopædia Britannica*:—

"We may remark that, although English writers have almost without exception confined themselves to the explanation of the methods of computing annuity tables and of determining from them the values of sums depending on life contingencies, the aid which this branch of economy derives from the general theory of probabilities is by no means confined to the consideration of such elementary questions. The number of observations necessary to inspire confidence in the tables, the extent to which risks may be safely undertaken, the comparative weights of different sets of observations, and the probable limits of departure from the average results of previous observations in a given number of future instances, are all questions of the utmost importance, which come within the scope of the calculus, and cannot in fact be justly appreciated by any other means."

In the concluding part of the latter extract we have, indeed, the real explanation of the formal and what I may venture to call the "wooden" cast that has been given to the subject by our standard writers already referred to: for we are to remember that,

though complete masters of their art as then understood, yet that they were all teachers or disciples of a school and of a day when the differential and integral calculus was but little employed by English writers on any branch of science. The omission of such processes has now, however, become the exception and not the rule. Thus, for instance, if we look into any of our modern treatises on mechanics, engineering, or navigation, all of them essentially practical subjects, we find every aid that the calculus can give or has a chance of giving sedulously pressed into the service. By these means the great discovery of Newton and Leibnitz is brought home—vicariously indeed, but still effectually—to the uses of the humblest mechanic, engineer, or mariner, whenever he has to avail himself of what can be done for him, by way of previous calculation, in guiding him to the simplest and most trustworthy results. Indeed, the modern improvements of the *Nautical Almanack* alone form at once a sufficient and striking illustration of what benefit can be achieved by the calculus in devising the best forms for practical computation. Whatever therefore may have been the opinion of our elder school of writers, I think the time has now come for our students when, as in other subjects, the more searching investigations of the calculus should also be brought to openly and commonly bear upon that of life contingencies.

Indeed, without this or some other extra aid, how is it possible for us to intelligibly explain to a modern public those differences of results in various Offices, which, when judged by a hypothetical standard of invariability, appear rather to proclaim the failure of all methods whatever, than to justify the indications of any particular one? So long as this diversity remains unexplained by having no proper limits assigned to it, so long assuredly may any amount of diversity appear justifiable to boards of management, and actuaries continue to be exposed to the risk of having their opinions only treated with respect when not obstructive of other money arrangements. That the calculus, especially considered as a calculus of averages, contains within itself the means of dealing with and explaining these diversities, has been too often asserted both by continental and native writers on probability, to be strengthened by mere reassertion on my part. But as I am not aware that any very ready example, in a professional sense, has been given of the sort of assistance to be derived by actuaries from this calculus, when treated as a calculus of averages, I shall beg leave to conclude this paper by offering at least one such illustration, hoping it may prove an incentive to other actuaries to look

further into the subject than perhaps they have hitherto done. Before, however, giving such an example, I should wish to state that I have purposely selected such an one as will show that I by no means pretend, as a practical man, that a more general study of the differential and integral calculus by actuaries would materially alter the external appearance of insurance results and rates, as at present accepted by the public. On the contrary, I believe that no actuarial theorizing would or ought to induce the public to be otherwise than mainly led by their own experience of the past, already somewhat extensive, and every day becoming more and more patent to themselves. But there is considerable difference, in a professional point of view, between venturing on general assumptions, however plausible, and the cautious adoption of approximations based upon elaborate investigations. Were then the calculus capable of no more than pointing out to us convenient approximations, and referring us to its own processes for its justification of them, it would still, I think, be an ally obviously well worthy of the actuary's seeking. It is to illustrate the calculus in this character that has decided the kind of example I have selected.

Example.—A hundred pounds has to be put out at compound interest for twenty years, at rates indefinitely fluctuating between 3 and 4 per cent. per annum. What is the general average of all the possible sums, even to infinity, to which the hundred pounds may be thus made to amount?

Putting this into the form of a definite integral, we have

$$\frac{1}{m^{n-1}}\int_a^{a+1}(b+cx)^n dx=\frac{1}{m^{n-1}}\,\frac{(b+c(a+1))^{n+1}-(b+ca)^{n+1}}{c(n+1)};$$

which, when $m=100$, $a=3$, $b=100$, $c=1$, and $n=20$, as in the case before us, becomes $\frac{1}{100^{19}}\left(\frac{104^{21}-103^{21}}{21}\right)=199{\cdot}2731$, which is the general average amount required.

Having thus determined what would appear as the more recondite question of the average *amount* of a sum at fluctuating rates of interest, it may be well, in order to show the ductility of the calculus when studied as an extensive system of averages, to also determine by its means the more simple question of what is the average rate of *interest* between 3 and 4 per cent., so obviously determinable by other means as $3\frac{1}{2}$? This extra illustration, however simple, is considered advisable; because there may be many minds, even in our own profession, so framed, that it is only by

treating well known examples, having obvious solutions by the current methods, that the reliability of any new method of solution is considered admissible by them in more difficult cases. To determine, then, the average rate of interest between 3 and 4 per cent. by means of the calculus, we have to consider the definite integral of

$$\int_a^{a+1} x dx = \frac{(a+1)^2 - a^2}{2};$$

which, when $a=3$, becomes $\frac{4^2-3^2}{2}=3\frac{1}{2}$; in exact equality with the obvious result of sheer mental arithmetic. I have already hinted that the first example is purposely chosen as one susceptible of an easy approximation, and such has just been portrayed: for if £100 be invested at $3\frac{1}{2}$ per cent. per annum for twenty years throughout, it will amount to £198·9789, a close approximation to the general average, or £199·2731, as determined by the calculus. But should we therefore be justified in saying that the £100 must necessarily be considered as having to be invested at $3\frac{1}{2}$ per cent. per annum throughout? Decidedly not; and the less so, because all the supposed advantages of such a misstatement are more readily obtained by adhering to the scientific truth, and saying that the proposed calculation, being really one of indefinite fluctuation, has been accordingly so dealt with, and the general average ascertained to be £199·2731; without, however, guaranteeing either that or any other as the precise result, that experience alone can determine. It is moreover manifest, that $3\frac{1}{2}$ as a rate of interest could not be connected as such with the average amount at the end of the term, for it is as obvious by common arithmetic as it is by the calculus that the accumulations between $3\frac{1}{2}$ and 4, considered as fixed quantities, would more than counterbalance those between 3 and $3\frac{1}{2}$: and hence, whether $3\frac{1}{2}$ would have to be considered as affording a good or bad approximation is not matter for assumption, but for demonstration; and it is precisely these demonstrations that are beyond the reach of the common methods.

The instances I have given will, I think, sufficiently portray, so far as isolated instances can do, the nature of the improvement I am advocating in life contingency calculations. It will be seen that, though I seek to deprive the prevailing system of its pretensions to an invariability that does not really belong to it, yet that at the same time I propose a similar equivalent, by the adoption of the calculus and its limits, to that which has already

been accepted, in place of a similarly false invariability, in other branches of natural philosophy. It is true, indeed, that before being exactly adapted to our wants as actuaries, the calculus must be moulded into one of averages; but this is a transformation so legitimate, that I consider no better method of studying the calculus exists even for the more general mathematical student.

In concluding this paper, I am perfectly well aware it is considered by many as dangerous, in an official and commercial sense, for any actuary to show he has been studying other books and productions than directories, prospectuses, and advertisements; but I trust that a better spirit is beginning to prevail, and that, within the walls of this Institute at least, any advocacy for the improvement of the theory of our subject will be immediately seen as also implying a desire to improve its practical aspects. Speaking for myself, I have long considered that the wants of the public are daily forcing upon actuaries the investigation of subjects which the incompleteness of the prevailing theory renders it too powerless to sufficiently grapple with; and it is the hope of exciting attention to this view of the matter that has induced me to offer the present paper. Considerable difficulty, as may be imagined from its tenor, has been felt in keeping it within due bounds; for, had examples of general limits been chosen, the subject in this form appears to be at present so little understood in its practical bearings, that at least the range of a lengthened essay, if not of a volume, might have been required to treat the matter with that fulness of illustration which the importance of it demands. It may therefore be allowable for me to attempt to reinforce the object of so circumscribed a paper by a general declaration on my part that, after having devoted considerable attention, and indeed some years, to the subject, I feel confident the proposed change from an invariable to a variable calculus as the basis of our calculations will be beneficial in every respect. We shall thereby be able to wholly dismiss the ancient doctrine of chances, with its fixed equalities of paper cards, wooden dice, and similar mechanical illustrations, and rely upon the more modern doctrine of probability, as the science of observation based upon experience. The actuarial adaptations of this doctrine, aided by the calculus, will assuredly ultimately bring a class of problems involving averages and their fluctuations within reach of our solutions which at present are merely statistically guessed at, even by the most experienced actuary, the most cautious finance minister, or the most learned political economist. To improve our own science, more-

over, is virtually a step towards the improvement of others, and thereby the better helps us to substantiate the claims of our studies to those designations of learned and liberal so duly prized by other professions.

POSTSCRIPT.—The writer is glad to avail himself of the interval between the reading and printing of his paper to state, that he does not by such paper claim for his views—as might perhaps hastily, without this disclaimer, be inferred—the merit of perfect originality as regards the proposed improvement of life contingency calculation. Lacroix has long since glanced at the differential and integral calculus as essentially a calculus of averages, and the calculus itself has already been often employed in connection with life contingencies in England, as by De Moivre, Waring, Young, Gompertz, Lubbock, De Morgan, Galloway, Edmonds, and indeed by the present writer himself, in his last publication on Life Contingency Tables. It is necessary further to remark, in the same spirit, that even the terms "true table" and "true rate of interest," though commonly used in the prevailing system, have also been frequently associated with the notion of a margin for fluctuations, or accompanied with the qualification that it is only by neglecting variations that the epithet of "true" becomes allowable, and that, if it be proposed to include such variations, that modification of the ordinary language should ensue. Reference may be made to Mr. Jellicoe's paper, in Vol I. p. 172 of the present *Journal,* for instances of this. It is, then, rather to excite renewed attention to the subject of variability, than to propose it as wholly new, that has been the writer's real aim; and he has accordingly treated the matter in the preceding paper in that mixed style of pleading and demonstration, as appearing to him the most suitable for such a purpose in its more general form. As however it may aid the illustrations already given in the paper itself, if a tabular form be presented, and may also tend to better satisfy many minds to whom tabular forms are more acceptable than even the most earnest disquisitions upon principles, such a table is now appended. It may be taken as a temporary specimen of the proposed improved manner of dealing with such subjects—without, however, the writer's wishing such table to be understood as having the exact form, even in his own opinion, that may ultimately be best adapted for the purpose under consideration.

Table of the Average Amounts of £100 at Fluctuating Rates of Interest.

Average Amounts of £100 at Compound Interest from 1 to 100 years, at rates fluctuating between the limits of 0 and 6 per cent. per annum.

N.B. The maximum rate of interest is taken at 6, rather than at 5, per cent.; because 5 per cent., if payable by half yearly, quarterly, monthly, or smaller instalments, can be made to exceed 5 per cent. per annum.

Term of Years.	Average Amount.	Term of Years.	Average. Amount	Term of Years.	Average Amount.	Term of Years.	Average Amount.
1	103·0000	26	235·9473	51	631·3104	76	1901·0467
2	106·1200	27	244·7433	52	658·4496	77	1990·5568
3	109·3654	28	253·9303	53	686·8833	78	2084·5473
4	112·7419	29	263·5273	54	716·6764	79	2183·2499
5	116·2553	30	273·5538	55	747·8971	80	2286·9085
6	119·9120	31	284·0306	56	780·6170	81	2395·7800
7	123·7183	32	294·9793	57	814·9117	82	2510·1350
8	127·6813	33	306·4428	58	850·8605	83	2630·2580
9	131·8080	34	318·3851	59	888·5470	84	2756·4491
10	136·1058	35	330·8913	60	928·0588	85	2889·0240
11	140·5829	36	343·9679	61	969·4884	86	3028·3153
12	145·2472	37	357·6426	62	1012·9330	87	3174·6731
13	150·1076	38	371·9448	63	1058·4948	88	3328·4664
14	155·1731	39	386·9049	64	1106·2813	89	3490·0835
15	160·4533	40	402·5553	65	1156·4058	90	3659·9337
16	165·9581	41	418·9299	66	1208·9873	91	3838·4479
17	171·6981	42	436·0641	67	1264·1512	92	4026·0800
18	177·6842	43	453·9955	68	1322·0292	93	4223·3082
19	183·9280	44	472·7634	69	1382·7602	94	4430·6361
20	190·4416	45	492·4090	70	1446·4903	95	4648·5943
21	197·2377	46	512·9758	71	1513·3730	96	4877·7418
22	204·3297	47	534·5094	72	1583·5703	97	5118·6675
23	211·7316	48	557·0580	73	1657·2523	98	5371·9917
24	219·4580	49	580·6718	74	1734·5982	99	5638·3681
25	227·5245	50	605·4040	75	1815·7968	100	5918·4853

Example.—The average amount of all the amounts possible, even to infinity, to which £100 can be made to accumulate in twenty years, at rates of interest fluctuating between 0 and 6 per cent. per annum, is £190·4416; as may be seen set forth in the table opposite to the term of 20 years.

When it is remembered that money is more likely to remain unproductive, or at 0 per cent., for short than for long periods, it is obvious that the relative effect of unproductiveness must be more operative when considering brief than enlarged cycles of finance. The preceding table, by its averages, properly represents this effect among others; and shows that while on the one hand the average amount at the end of the first period or year from the original times of the deposits may be taken as sufficiently defined by the common mean rate of interest between the limits, or in the present

case by 3 per cent., yet that, on the other hand, the period of a century is allied by its average amount, in connection with the same limits, to the amount productive by a uniform rate throughout of about 4 per cent. (4·165). To deal with such wide limits as from 0 to 6, and with such durations as a century, is obviously to strain the calculus to its utmost; but even in this extreme state it will be seen to keep closely attendant upon the incidents of practical insurance, for it certainly appears consistent with even popular justice that, as a matter of calculation, those who may remain longest insured should be also rated as those to be relatively assigned the higher ratios in the general appropriation of accumulations of interest.

The table has been virtually calculated by the aid of the same definite integral as that given in the paper, viz.—

$$\frac{1}{m^{n-1}}\int_a^{a+g}(b+cx)^n dx = \frac{1}{m^{n-1}}\,\frac{(b+c(a+g))^{n+1}-(b+ca)^{n+1}}{gc(n+1)};$$

in which n varies by units from 1 to 100; a varies indefinitely to and from $a+g$, or from 0 to 6; $b=100$; $m=100$; $c=1$; and $g=6$. Or, putting such expressions into the form of a rule, we have the following extremely simple one whenever 0 is the lower limit:—

Subtract £100 from its amount, when improved for the whole term and one year beyond, at the maximum rate of interest considered as uniform as by the common tables. Divide the remainder by the product of such maximum rate and the number of years including the year beyond, and the quotient will be the average amount required.

Example for Twenty Years.—£100, put out at 6 per cent. per annum uniform interest for twenty years and one year beyond, will amount by the ordinary tables (Smart's) to £339·95636; from which if the £100 be subtracted, the remainder is £239·95636; which, divided by 6 times 20 and the year beyond, or 126, gives a quotient of £190·4416, which is the average amount required between the assigned limits in conformity with the result as given by the table in question.

The reasons for the trustworthiness of the rule can of course only be explained by aid of the calculus itself, or by some allied process of reasoning which it would be here out of place to dilate upon.